I cannot allow the present communication to appear in these columns without again recording my conviction that the writer is the most profound of living thinkers upon Darwinian topics, and that the generalizations which have been reached by his twenty years of thought are of more importance to the theory of evolution than any that have been published during the post-Darwinian period. GEORGE J. ROMANES.

London, March 10.

I FOLLOW Prof. Lankester in the use of bionomics to designate the science treating of the relations of species to species. If the theory of evolution is true, bionomics should treat of the origin, not only of species, but of genera, and the higher groups in which the organic world now exists.

In his very suggestive review of "Darwinism," by Mr. A. R. Wallace, in NATURE of October 10, 1889 (p. 566), Prof. Lankester refers to "his (Mr. Wallace's) theory of the importance of the principle of 'like to like' in the segregation of varieties, and the consequent development of new species." Prof. Lankester has here alluded to a principle which I consider more fundamental than natural selection, in that it not only explains whatever influence natural selection has in the formation of new species, but also indicates combinations of causes that may produce new species without the aid of diversity of natural selection. The form of like to like which Mr. Wallace discusses is "the constant preference of animals for their like, even in the case of slightly different varieties of the same species," which is considered not as an independent cause of divergence, but as producing isolation which facilitates the action of natural selection. If he had recognized this principle, which he calls selective association, as capable of producing in one phase of its action sexual and social segregation, and in another phase sexual and social selection, he would perhaps have seen that its power to produce divergence does not depend on its being aided by natural selection.

Mr. Wallace's view is very clearly expressed in the following passages, though I find other passages which lead me to think that the chief reason he does not recognize segregation as the fundamental principle in divergence is that he has not observed its relations to the principle of like to like. He says:—"A great body of facts on the one hand, and some weighty arguments on the other, alike prove that specific characters have been, and could only have been, developed and fixed by natural selection because of their utility" ("Darwinism," p. 142). "Most writers on the subject consider the isolation of a portion of a species a very important factor in the formation of new species, while others maintain it to be absolutely essential. This latter view has arisen from an exaggerated opinion as to the power of intercrossing to keep down any variety or incipient species, and merge it in the parent stock" ("Darwinism," p. 144).

I think we shall reach a more consistent and complete apprehension of the subject by starting with the fundamental laws of heredity, and refusing to admit any assumption that is opposed to these principles, till sufficient reasons have been given. Laws which have been established by thousands of years of experiment in domesticating plants and animals, should be, it seems to me, consistently applied to the general theory of evolution. For example, if in the case of domesticated animals, "it is only by isolation and pure breeding that any specially desired qualities can be increased by selection" (see "Darwinism," p. 99), why is not the same condition equally essential in the formation of natural varieties and species? If in our experiments we find that careful *selection* of divergent variations of one stock does not result in increasingly divergent varieties *unless free crossing between the varieties is prevented*, why should it be considered an exaggeration to hold that in wild species "the power of intercrossing to keep down any variety or incipient species, and merge it in the parent stock," is the same. Experience shows that *segregation, which is the bringing of like to like in groups that are prevented from crossing*, is the fundamental principle in the divergence of the various forms of a given stock, rather than *selection, which is like to like through the prevention of certain forms from propagating*; and I think we introduce confusion, perplexity, and a network of inconsistencies into our exposition of the subject, whenever we assume that the latter is the fundamental factor, and especially when we assume that it can produce divergence without the co-operation of any cause of *segregation* dividing the forms that propagate into two or more groups of similars, or when we assume that segregation and divergence cannot be produced without the aid of diverse forms of selection in the different groups. The theory of divergence through segregation states the principle through which natural selection becomes a factor promoting sometimes the stability and sometimes the transformation of types, but never producing *divergent* transformation except as it co-operates with some form of isolation in producing segregation; and it maintains that, whenever variations whose ancestors have freely intergenerated are from any combination of causes subjected to persistent and cumulative forms of segregation, divergence more or less pronounced must be the result. The laws of heredity on which this principle rests may be given in the three following statements:—

(1) Unlike to unlike, or the removal of segregating influences, is a principle that results either in extinction through failure to propagate, or in the breaking down of divergences through free crossing.

(2) Like to like, when the individuals of each intergenerating group represent the average character of the group, is a principle through which the stability of existing types is promoted.

(3) Like to like, when the individuals of each group represent other than the average character of the group, is a principle through which the transformation of types is effected.

In my paper on "Divergent Evolution" (Linn. Soc. Journ., Zoology, vol. xx. pp. 189–274), I pointed out that sexual and social instincts often conspire together to bring like to like in groups that do not cross, and that in such cases there will be divergence even when there is no diversity of natural selection in the different groups, as, for example, when the different groups occupy the same area, and are guided by the same habits in their use of the environment. There is reason to believe that under such circumstances divergence often arises somewhat in the following way. Local segregation of a partial nature results in some diversity of colour or in some peculiar development of accessory plumes, and through the principle of social segregation, which leads animals to prefer to associate with those whose appearance has become familiar to them, the variation is prevented from being submerged by intercrossing. There next arises a double process of sexual and social selection, whereby both the peculiar external character and the internal instinct that leads those thus characterized to associate together are intensified. The instinct is intensified, because any member of the community that is deficient in the desire to keep with companions of that kind will stray away and fail of breeding with the rest. This process I call social selection. The peculiarity of colour or plumage is preserved and accumulated, because any individual deficient in the characteristic is less likely to succeed in pairing and leaving progeny. This latter process is sexual selection. It can hardly be questioned that both these principles are operative in producing permanent varieties and initial species; and in the circumstances I have supposed, I do not see how the process can be attributed to natural selection. Varieties thus segregated may often develop divergent habits in their use of the environment, resulting in divergent forms of natural selection, and producing additional changes; but so long as their habits of using the environment remain unchanged, their divergencies cannot be due to natural selection.

Mr. Wallace's very interesting section on "Colour as a Means of Recognition," taken in connection with the section on "Selective Association," already referred to, and another on "Sexual Characters due to Natural Selection," offers an explanation of "the curious fact that prominent differences of colour often distinguish species otherwise very closely allied to each other" (p. 226). His exposition differs from mine in that he denies the influence of sexual selection, and attributes the whole process to natural selection, on the ground that "means of easy recognition must be of vital importance" (p. 217). The reasoning, however, seems to me to be defective, because the general necessity for means of easy recognition is taken as equivalent to the necessity for a specialization of recognition marks that shall enable the different varieties to avoid crossing. In the cases I am considering, there is, however, no advantage in the separate breeding of the different varieties, and even in cases where there is such an advantage (as there would be if the variety had habits enabling it to escape from competition with the parent stock, but only partially preventing it from crossing with the same), it does not appear how this advantage can prevent the individual that is defective in the special colouring from following and associating with those that are more clearly marked. The significant part of the process in the development of recognition marks must be in the failure of such individuals to secure mates, which is sexual selection; or in the unwillingness of the

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community to tolerate the company of such, which might be called social selection.

It is often assumed by writers on evolution that permanent differences in the methods in which a life-preserving function is performed are necessarily useful differences. That this is not so may be shown by an illustration drawn from the methods of language. The general usefulness of language is most apparent, and it is certain that some of the laws of linguistic development are determined by a principle which may be called "the survival of the fittest;" but it is equally certain that all the divergences which separate languages are not useful divergences. That one race of men should count by tens and another by twenties is not determined by differences in the environments of the races, or by any advantage derived from the difference in the methods. So easy recognition of other members of the species is of the highest importance for every species; but difference in "recognition marks" in portions of a species separated in different districts of the same environment is no advantage. Under the same conditions, habits of feeding may become divergent; but, since any new habit that may be found advantageous in one district would be of equal advantage in the other district, the divergence must be attributed to some initial difference in the two portions of the species.

I have recently observed that, of two closely allied species of flat-fish found on the coasts of Japan, one always has its eyes on the right side, and the other always on the left. As either arrangement would be equally useful in the environment of either species, the divergence cannot be considered advantageous.

Osaka, Japan. JOHN T. GULICK.

Self-Colonization of the Coco-nut Palm.

THE question whether the coco-nut palm is capable of establishing itself on oceanic islands, or other shores for the matter of that, from seed cast ashore, was long doubted; and if the recent evidence collected by Prof. Moseley, Mr. H. O. Forbes, and Dr. Guppy, together with the general distribution of the palm, be not sufficient to convince the most sceptical person on this point, there is now absolutely incontrovertible evidence that it is capable of doing so, even under apparently very unfavourable conditions.

In the current volume of NATURE (p. 276) Captain Wharton describes the newly-raised Falcon Island in the Pacific; and in the last part of the Proceedings of the Royal Geographical Society, Mr. J. J. Lister gives an account of the natural history of the island. From this interesting contribution to the sources of insular floras we learn that he found two young coco-nut palms, not in a very flourishing condition, it is true; but they were there, and had evidently obtained a footing unaided by man. There were also a grass, a leguminous plant, and a young candle-nut (Aleurites), on this new volcanic island—a very good start under the circumstances, and suggestive of what might happen in the course of centuries.

W. BOTTING HEMSLEY.

On Certain Devonian Plants from Scotland.

I AM indebted to Mr. James Reid, of Allan House, Blairgowrie, Scotland, for the opportunity to examine a collection of fossil plants obtained by him from the Old Red Sandstone of Murthly and Blairgowrie in Perthshire, some of which have been noticed by Dr. Geikie in his "Text-book of Geology."

The collection is remarkable for the striking resemblance of the matrix and the contained vegetable debris to those of the lower part of the Gaspé sandstones of Logan, and the species of plants are, so far as can be determined, the same.[1]

Psilophyton princeps largely predominates, as in Gaspé, and is represented by a profusion of fragments of stems and branches, and more rarely by specimens of the rhizoma and of the sporocarps. *P. robustius* is represented by fragments of stems, but is less abundant, and *Arthrostigma gracile* by some portions of stems. On the whole the assemblage is exactly those of the sandstone beds of the lower division of the Gaspé sandstones. There is nothing distinctively Upper Devonian in the collection.

The collection also contains two slabs of dark-coloured sandstone from Caithness, one of which contains what appears to be a fern stipe similar to those of the genus *Rhodea*. Another shows a remarkable plant having apparently a short stem giving origin to a quantity of crowded leaves which are long, narrow, and parallel-sided, and show only a very faint linear striation. This plant is identical both in the form and arrangement of the leaves with that found in the Devonian of Canada, and which I have named *Cordaites angustifolia*. I have, however, already stated in my Reports on the Flora of the Erian of Canada (Geological Survey of Canada, 1871 and 1882), that I do not consider this plant as closely related to the true Cordaites, and that I have not changed the generic name merely because I am still in doubt as to the actual affinities of the plant. Mr. Reid's specimens would rather tend to the belief that it was, as I have already suggested in the reports above cited, a Zostera-like plant growing in tufts at the bottom of water.

Some of the sandstone slabs from Murthly contain specimens of rounded objects referable to *Pachytheca* (Hooker), a genus of uncertain affinities but characteristic of Silurian and Lower Devonian beds on both sides of the Atlantic. One of these is perfectly spherical with a shining surface, and 2·75 mm. in diameter, the others have been broken so as to show a central cavity or nucleus about 1 mm. in diameter, and with a thick carbonaceous wall partly pyritised and showing obscure radiating fibres. Prof. Penhallow, of McGill University, has kindly examined these, and has compared them with slices of Pachytheca from the Wenlock limestone, kindly communicated by Mr. Barber, of Cambridge, and with specimens presented by Prof. Hicks from the Silurian of Corwen and with specimens in the author's collection from the Silurian of Cape Bon Ami; and also with the excellent figures in Mr. Barber's paper in the *Annals of Botany*. He has not been able, however, to arrive at any conclusions beyond the probable general similarity in structure of the various forms, which may, however, as Mr. Barber suggests, have differed in their nature and origin. The only thing certain at present seems to be that these puzzling organisms had a thicker outer coat of radiating fibres, and of so great density that it was less liable to compression than the other vegetable tissues with which it is associated.

A few small specimens sent more recently by Mr. Reid contain some curious but not very intelligible objects from the same beds. One is a stem coiled at the end very closely in a circinate manner. In form it resembles the circinate vernation of *Psilophyton princeps*, but is much larger. It may belong to *P. robustius*, or possibly to a fern, but is too obscure for certain determination. Several others appear to represent flattened fruits or sporangia of obovate form and of large size. One has a stalk attached with what seems a rudiment of a bract, and another shows obscure indications of having contained round or disk-shaped bodies about 2 mm. in diameter. All show minute longitudinal striation. I have not previously met with bodies of this kind in the Devonian, and can only suggest that they may represent the fructification of some unknown plant, possibly that to which Pachytheca belonged. J. WM. DAWSON.

Montreal, March 5.

Exact Thermometry.

I AM glad to observe that Prof. Sydney Young and myself are now in substantial agreement as regards the tension theory of the ascent of the zero in thermometers, and approximately in agreement as regards the actual cause of the ascent in the neighbourhood of the ordinary temperature.

Some time ago, in connection with an investigation of melting-point, I devoted three years to an examination of the properties of the mercurial thermometer. Among other conclusions which then seemed to me probable, the application of the known plasticity of glass under pressure to account for the enormous ascent (in lead-glass) of the zero at high temperatures appeared of some value. I have never advanced it as a mature theory, and am perfectly open to correction on the subject; but neither Prof. Crafts (with whom I at that time discussed the matter), nor any subsequent experimenter, has submitted the suggestion to a crucial examination.

Prof. Young's experiments (NATURE, March 27, p. 489) are very interesting as far as they go; but the kind of glass of which his thermometers are constructed is not that which brings out the peculiarities of the material in their most striking development. This, indeed, has long been known. It may well be that, in German soda-glass, the plasticity is masked by a preponderating tendency of the harder or more crystalline silicates of the bulb to set. Much could be done towards settling the question as to plasticity, if three thermometers of lead-glass—one vacuous,

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[1] See papers by the author, Journal Geol. Society, London, 1859, and Proceedings Geol. Society, Edinburgh, 1877.

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